

BEHAVIORAL ECONOMICS

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Methods: Theory. Simple models of psychological phenomena

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1 INTRODUCTION

In recent years, behavioral economics has emerged as a *bona fide* subdiscipline of economics.ⁱ Because behavioral economics in certain ways represents a sharp departure from mainstream – that is, neoclassical – economics, it raises a number of questions of a philosophical, methodological and historical nature. Yet, to date, it has not received the attention it deserves from historians and philosophers of science.ⁱⁱ In this chapter, we take some initial steps to address this deficiency. Our purpose is to shed light on (a) the nature and historical origins of behavioral economics as a field, (b) its main results and their interpretation, (c) the methods used by its practitioners, (d) its relationship to traditional economics as well as to other emerging subdisciplines such as neuroeconomics, and (e) some of its philosophical and methodological underpinnings. We make no claim, of course, to settle or even raise all issues raised by the emergence of behavioral economics; we do want to go some way toward figuring out what those issues are.

The term “behavioral economics” was in use as early as 1958 (cf. Johnson 1958; Boulding [1958] 1961, 21). These days, as it is typically employed, “behavioral economics” refers to the attempt to increase the explanatory and predictive power of economic theory by providing it with more psychologically plausible foundations (Camerer and Loewenstein 2003, 3; cf. Weber and Dawes 2005, 91). Notice that behavioral economics so defined has little to do with behaviorism; in fact the historical roots of behavioral economics can be traced to cognitive psychology, which emerged in direct opposition to behaviorism (see section 2). The modifier “behavioral” – which is sometimes criticized for being redundant on the grounds that all economics is or should be about behavior – stems from the origins of behavioral economics in

behavioral decision research (see section 4.1). Behavioral economists do not deny that there may be much to learn from sociology, anthropology and other neighboring fields. However, most of the work characterized as behavioral economics these days – and virtually all the work reviewed here – is, in fact, inspired by psychology. A separate subfield that draws on sociology, and which is sometimes referred to as “socioeconomics,” has coalesced around a different set of researchers and journals.

Our main thesis is that behavioral economics should be seen as a branch of cognitive science. Thus, we agree with Russell Sage Foundation president Eric Wanner, who has helped fund research in behavioral economics since the mid-1980s, and who has been instrumental in the establishment of behavioral economics as an independent subdiscipline. Wanner describes behavioral economics as an application of cognitive science to the realm of economic decision-making. “The field is misnamed – it should have been called *cognitive economics*,” he says. “We weren’t brave enough” (quoted in Lambert 2006: 52, italics in original). Drawing on Howard Gardner’s work on the origins and nature of cognitive science – especially his classic *The Mind’s New Science: A history of the cognitive revolution* ([1985] 1987)ⁱⁱⁱ – we will argue that seeing behavioral economics as a branch of cognitive science is eminently useful for understanding both the historical origins, nature, strengths and weaknesses of behavioral economics.

2 THE INTELLECTUAL BACKDROP

When cognitive science emerged in the 1940s and 50s, it did so in opposition to behaviorism and associated doctrines, including logical positivism and verificationism (Gardner 1987, 10 *ff*). In Gardner’s words, scientists of this era had developed “a growing awareness ... that adherence to behaviorist canons was making a scientific study of the mind impossible” (Gardner 1987, 12).

This was so in large part because behaviorism and associated doctrines “eschewed entities (like concepts and ideas) that could not be readily observed and reliably measured” (Gardner 1987, 15). Here, we will argue that something very similar is true for behavioral economics. Behavioral economics emerged in opposition to neoclassical economics, which was heavily inspired by behaviorism and associated doctrines, including verificationism and operationalism. In particular, behavioral economics emerged in reaction to the notion, held by many neoclassical economists, that social and behavioral science should avoid reference to entities (like cognitive and affective states) that cannot be directly observed.

Our examination of the origins and development of neoclassical economics serves two main purposes. First, because behavioral economics largely emerged in reaction to neoclassical economics, a historical excursion allows us to paint a fuller picture of the views against which behavioral economics reacted. Second, because most of the critics of behavioral economics have a neoclassical background, it allows us to achieve a better understanding of their criticism. In passing, this section is also intended to illustrate that the project to rid economics of its ties to psychology (a project described in section 2.2) is relatively modern; in fact, we will argue, both classical and early neoclassical economists were deeply interested in the psychological underpinnings of economic behavior. Our exposition largely follows that of Michael Mandler (1999), who divided the history of modern economics into three main periods: *classical*, *early neoclassical*, and *postwar neoclassical* (Mandler 1999, 3). Like all divisions of this sort, Mandler’s is imperfect – for one thing, authors representative of e.g. postwar theory may have published before the war – but for present purposes it is good enough.

2.1 *Classical and early neoclassical economics*

Before the emergence of behaviorism during the first decades of the twentieth century, psychologists were largely comfortable with talking about to mental states and other unobservables. “At the turn of the century,” Gardner writes, “investigators had been addressing the key issues of mental life: thinking, problem solving, the nature of consciousness” (Gardner 1987, 11). Similarly, as we will see, classical and early neoclassical economists made frequent reference to cognitive and affective states. Their conception of human nature – and therefore human decision making – was often relatively sophisticated, and in many cases inspired by developments in psychology. These facts are important, because there are many misconceptions about the views of human nature implicit (or explicit) in the writings especially of the classical economists of the 18th century (including Adam Smith). Perhaps because they are interpreted in light of modern neoclassical economics, these economists are often falsely attributed a particularly simple psychology, according to which people consistently, everywhere and always, pursue their own self-interest narrowly construed.

The actual views of the classical economists could hardly be more different. Regarding the nature of human ends, for example, Smith wrote: “How selfish soever man may be supposed, there are evidently some principles in his nature, which interest him in the fortune of others, and render their happiness necessary to him, though he derives nothing from it except the pleasure of seeing it” (Smith [1759] 2002, 11). Similarly, regarding people’s rationality, or utility-promoting behavior, Smith wrote:

How many people ruin themselves by laying out money on trinkets of frivolous utility? What pleases these lovers of toys is not so much the utility, as the aptness of the machines which are fitted to promote it. All their pockets are stuffed with little conveniences ... of which the whole utility is certainly not worth the fatigue of bearing the burden (Smith [1759] 2002, 211).

Whatever the exact implications of these quotes, they show rather clearly that Smith did not have the simple-minded view of human nature that some would have it.

Smith does not, of course, have a theory of decision in the modern sense, but he did express a vision of human nature (and therefore human action) which is remarkably multifaceted. As Mark Perlman and Charles McCann (1998) describe it:

Smith's *homo economicus* ... was a man with a temporal sense, a man with loyalties, a man who clearly understood that he was part of a larger social collective. What Smith's man wanted and needed was the responsibility for making his own decisions and accepting the consequences of those decisions. This responsibility had to be understood as existing in concert with the twin principles of self-love and sympathy, for all were combined in the Smithian calculus. In brief, in modern parlance what was to be maximized by Smith's man was the right of self-determination, while still allowing a place for both moral and social sensibilities and even expressions of altruism (Perlman and McCann 1998, 239).

The exact details of Smith's conception of human nature are contested (cf. Otteson 2002; Schliesser 2005). It should be clear, however, that Smith – like his contemporary David Hume – was deeply interested in the psychological underpinnings of human behavior. Moreover, arguably, Smith's views about human psychology were not incidental to his more purely economic work, and may have had an important impact on them (cf. Davis 2003, 270).

Reading the classical economists' philosophical and economic psychology, several contemporary authors have gone so far as suggesting that Hume and Smith in fact identified and discussed some of the phenomena that now occupy behavioral economists. Thus, Ignacio Palacios-Huerta argues that both Hume and Smith analyzed dynamically inconsistent behavior, and that “their analyses of this behavior remain novel” (Palacios-Huerta 2003, 243). Similarly, Nava Ashraf, Colin F. Camerer and George Loewenstein (2005, 140) argue that Smith's work “is not only packed with insights that presage developments in contemporary behavioral economics,

but also with promising leads that have yet to be pursued.”^{iv} These insights include phenomena now referred to as loss aversion, overconfidence, social preferences, and more.

Early neoclassical economics is best characterized by the work of William Stanley Jevons. These economists, including Jevons, explicitly built their economics on the foundation of hedonic psychology, that is, an account of individual behavior according to which individuals seek to maximize pleasure and minimize pain. In Jevons’ words: “Pleasure and pain are undoubtedly the ultimate objects of the Calculus of Economics. To satisfy our wants to the utmost with the least effort ... in other words, to *maximize pleasure*, is the problem of Economics” (Jevons [1871] 1965, 37). The early neoclassical economists were inspired by Bentham, who wrote: “Nature has placed mankind under the governance of two sovereign masters, *pain* and *pleasure*.... They govern us in all we do, in all we say, in all we think” (Bentham [1823] 1996, 11, italics in original). These economists understood utility in terms of conscious experience like pleasure or happiness. As Jevons puts it: “Utility [arising from any commodity] must be considered as measured by, or even as actually identical with, the addition made to a person’s happiness” (Jevons [1871] 1965, 45).

When it came to welfare economics, early neoclassical economists were unabashed utilitarians. A. C. Pigou, author of *The Economics of Welfare* ([1920] 1952) and commonly considered the father of welfare economics, went to great lengths exploring and measuring “total welfare” ([1920] 1952). Early neoclassical economists like Pigou believed that welfare or utility could meaningfully be aggregated across individuals, and that one state was superior to another if total welfare was greater in the former than in the latter (Mandler 1999, 4). Of course, welfare economists of the time shared the focus on conscious experience. As Pigou put it, “the elements of welfare are states of consciousness and, perhaps, their relations” (Pigou [1920] 1952, 10).

Mandler argues that the hedonic foundations of economics – and especially the assumption that people maximize pleasure – conferred multiple advantages. First, hedonics came with an account of deliberation, according to which individuals weigh the pleasure and pain that would result from various actions and choose the one they perceive as leading to the greatest balance of pleasure over pain (Mandler 1999, 76). Second, hedonics provided a rationale for several critical assumptions, such as the completeness and transitivity of the preference relation, and (given the further assumptions of separability and diminishing marginal utility) the convexity of indifference curves (Mandler 1999, 76-77). Third, “the early neoclassical account of rational deliberation allowed for a rich description of irrational (‘incorrect’) behavior” (Mandler 1999, 77). Hedonic psychology permits people to act irrationally because, for example, they fail to properly anticipate the pleasure resulting from certain actions, or because (in the intertemporal context) they fail to properly take future pleasure into account in their deliberations (see, e.g., Loewenstein, O'Donoghue & Rabin, 2003). In sum, the assumption that people maximize pleasure could explain *both* why preferences in general are transitive, etc., and why people sometimes act irrationally.

The identification of utility with conscious experience had important methodological implications. Because it was assumed that individuals have direct access to their conscious experience, many economists defended the principles of hedonic psychology on the basis of their introspective self-evidence alone. Thus, John E. Cairnes wrote: “*The economist starts with a knowledge of ultimate causes*. He is already, at the outset of his enterprise, in the position which the physicist only attains after ages of laborious research” (Cairnes [1888] 1965, 87, italics in original). The reason, Cairnes continues, is that “we have, or may have if we choose to turn our attention to the subject, direct knowledge of these causes in our consciousness of what passes in

our own minds” (Cairnes 1965, 88). Because of their commitment to introspection, in conjunction with the belief that introspection supported the principles of hedonic psychology, early neoclassical economists like Cairnes saw little reason to explore alternative methods to confirm the adequacy of the foundations of their economics. The heavy reliance on introspection was not unique for the economists. As Gardner put it: “Unfortunately the scientific method favored by most researchers at the time was introspection” (Gardner 1987, 11).

2.2 *Postwar neoclassical theory*

The emergence of behaviorism – marked by the appearance of John B. Watson’s article ‘Psychology as the Behaviorist Views it’ (1913) – included an attack on both the heavy reliance on introspection and the references to mental states. In Gardner’s words:

The behaviorists put forth two related propositions. First of all, those researchers interested in a science of behavior ought to restrict themselves strictly to public methods of observations, which any scientist could apply and quantify... Second, those interested in a science of behavior ought to focus exclusively on *behavior*: researchers ought assiduously to eschew such topics as mind, thinking, or imagination and such concepts as plans, desires, or intentions (Gardner 1987, 11, italics in original).

The two propositions are clearly present in the writings of the postwar neoclassical economists as well. The transition from early to postwar neoclassical theory, although inspired by earlier work (e.g. Pareto [1906] 1971; see Bruni & Sugden, 2007), took place over the course of some 20 years, from the mid-1930’s to the mid-50’s (Mandler 1999, 8). As we will see, postwar neoclassical economists wanted to gain distance from psychology of all kinds, objected to the notion that economics should make reference to conscious states, and rejected the idea that introspection was a scientifically acceptable means to explore such states.

Postwar neoclassical economists were motivated by a variety of considerations. Many of them appear to have been directly inspired by the methodological strictures of logical positivism in philosophy, behaviorism in psychology, and operationalism in physics (cf. Lewin 1996). Moreover, some economists had grown disappointed with the meager results of early neoclassicism in terms of generating theories with predictive power. In a tart critique of Cairnes ([1888] 1965; quoted above), T. W. Hutchison remarked:

It is possibly very encouraging for the economist to hear that compared with the natural scientist the psychological method saves him “ages of laborious research,” but it is curious and a pity that this huge start has not enabled him to formulate any considerable body of reliable prognoses such as the natural sciences have managed to achieve (Hutchison 1938, 132).

Thus, postwar neoclassical economists set out to put their discipline on firmer methodological ground, and at the same time to improve the predictive power of their theories.

According to the postwar neoclassical view, or *ordinalism* as it is often called (cf. Hicks 1975), the fundamental assumption is that people have preferences. As Lionel Robbins, author of the spectacularly influential *An Essay on the Nature and Significance of Economic Science* ([1932] 1984) wrote, “all that is assumed ... is that different goods have different uses and that these different uses have different significances for action such that in a given situation one use will be preferred before another and one good before another” (Robbins [1932] 1984, 85-86). Thus, a person’s preference ordering just represents his or her ranking of whatever options are available, nothing more, nothing less. As Philip H. Wicksteed expresses it:

By a man’s “scale of preferences” or “relative scale,” then, we must henceforth understand the whole register of the terms on which (wisely or foolishly, consistently or inconsistently, deliberately, impulsively or by inertia, to his future satisfaction or to his future regret) he will, if he gets the chance, accept or reject this or that alternative (Wicksteed [1910] 1967, 36).

By using “preference” rather than “utility” as the primitive concept, postwar neoclassical economists explicitly intended to rid economics of its ties to psychology – hedonic and otherwise. As Robbins wrote, neoclassical economic theory “is capable of being set out and defended in absolutely non-hedonistic terms” and has no “essential connection with psychological hedonism, or for that matter with any other brand of *Fach-Psychologie*” (Robbins 1984, 85).

It is important to notice that postwar economists did not deny that people might be motivated by pleasure, pain and/or other mental states. As J. R. Hicks (1946) puts it: “Now of course this does not mean that if any one has any other ground for supposing that there exists some suitable quantitative measure of utility, or satisfaction, or desiredness, there is anything in the above argument to set against it” (Hicks 1946, 18). Instead, postwar economists chose to remain agnostic about questions of motivation, preference formation, and choice. Moreover, they often argued that such issues were outside the scope of economics. Thus, for example, Robbins writes: “Why the human animal attaches particular values in this sense to particular things, is a question which we do not discuss. That is quite properly a question for psychologists or perhaps even physiologists” (Robbins 1984, 86).

Because of its agnosticism about the psychological underpinnings of human behavior, ordinalism is often described as less committal than classical and early neoclassical economics. Hence: “Since preference orderings do not presuppose cardinal judgments of satisfaction intensity, and since agents may well form their preference rankings through entirely nonhedonistic means, ordinalism is more general than a utility- or pleasure-based approach” (Mandler 1999, 5). While the early neoclassical economists made assumptions about individual psychology – for instance, how feelings of pleasure and pain change as a result of consumption –

and deduced the properties of preference – e.g. the claim that preferences must be transitive – postwar neoclassical simply treats the transitivity of preferences as axiomatic (Mandler 1999, 5).

While ordinalist economists are happy to talk about utility, they use the term in a different way. They take utility to be an index or a measure of preference satisfaction. In Mandler's words:

An agent's ordinal preference for option x over y simply indicates the relative ordering of the options and not that x delivers some specified multiple of y 's pleasure or even that pleasure is the motivating factor. Indeed, contemporary economics relinquishes any attempt to specify the motives underlying choice. Preference itself is now the primitive element of consumer theory; there is no need to peer into agents' psyches (Mandler 1999, 78).

In this view, to say that the utility of x is greater than that of y for person p , is just to say that p prefers x over y . In brief, utilities have nothing to do with pleasure, pain, or any other psychological or motivational state. Postwar economists differ in their understanding of the concept of "preference," however. According to some accounts – like Paul Samuelson's (1948) Revealed Preference Theory – preferences are identified with observable choices. According to others – like that of Robbins – preferences are not identified with observable choices but are nevertheless closely linked. The link, whatever its exact nature, is such that choices mirror preferences, so that choices have the same properties and preferences, and so that choice data can be used to infer preference orderings (see Robbins [1932] 1984, 87-88).

Ordinalism, obviously, has implications for welfare economics (see Hicks 1975). First, mental state accounts of welfare (according to which welfare is a matter of happiness, pleasure, or the like) gave way to preference satisfaction accounts (according to which welfare is a matter of preference satisfaction). Moreover, the utilitarian welfare criterion was jettisoned in favor of the Pareto criterion, which says that one state is superior to another if at least one individual is

better off, and nobody is worse off, in the former than in the latter. The Pareto criterion was supposed to allow economists to dispense economic advice without requiring the aggregation of utilities or interpersonal welfare comparisons (Mandler 1999, 6). However, realizing that the Pareto criterion is very strong – few changes in e.g. economic policy are true Pareto improvement – postwar neoclassical economists have suggested a series of weaker conditions, including potential Pareto improvements, etc..

Ordinalism also has methodological implications. As a result of the rejection of introspection, postwar neoclassical economists adopted the belief that the only valid method to collect information about preferences is to study market transactions or other observable choices. This belief, as we have seen, was shared with the behaviorists. As Gardner puts it, behaviorists tolerated “[no] subjective ruminations or private introspection: if a discipline were to be a science, its elements should be as observable as the physicists’ cloud chamber or the chemist’s flask” (Gardner 1987, 11). Typical of the attitude of behaviorists was Edward C. Tolman’s famous statement that “everything important in psychology ... can be investigated in essence through the continued experimental and theoretical analysis of the determiners of rat behavior at a choice point in a maze” (Tolman 1938, 34). Commenting on the economists’ version of this belief, which remains strong, Amartya Sen writes:

Choice is seen as solid information, whereas introspection is not open to observation.... Much of economic theory seems to be concerned with strong, silent men who never speak! One has to sneak in behind them to see what they are doing in the market, etc., and deduce from it what they prefer, what makes them better off, what they think is right, and so on (Sen 1982, 9).

Similarly: “Much of the empirical work on preference patterns [and therefore welfare] seems to be based on the conviction that [non-verbal] behaviour is the only source of information on a person’s preferences” (Sen 1982, 71).^v

In brief, postwar neoclassical economics represents a sharp break with the classical and early neoclassical tradition. In the process of rendering economics more consistent with contemporary methodological strictures, and to improve the predictive power of the theory, postwar theorists aspired to sever all ties with psychology, hedonic and otherwise. As a result, they acquired a theory of great generality, whose adequacy does not hinge on the plausibility of any particular account of human behavior. Meanwhile, several advantages of early neoclassical theory were lost. First, postwar theory (unlike early neoclassical theory) did not come with an account of deliberation. Thus postwar neoclassical economists are unable to say anything about how preferences are formed. Second, and relatedly, postwar theory did not provide any theoretical basis for the assumptions underlying the preference account of behavior. As Mandler put it: “Lacking psychological foundations, the axioms of preference theory instead persist as primitives, unexplained and unjustified” (Mandler 1999, 66). Finally, and perhaps most importantly, when it comes to interactions between economics and psychology, postwar theorists lost the theoretical resources to describe irrational behavior; insofar as the theory can describe behavior at all – that is, insofar as behavior is consistent – the theory necessarily describes behavior as rational. Similarly, insofar as welfare or wellbeing is understood in terms of the satisfaction of the person’s preferences, the theory necessarily describes any voluntary action as, at least *ex ante*, promoting his or her welfare or wellbeing.

2.3 Discussion

In this section we have tried to paint a fuller picture of the historical background from which behavioral economics emerged, from the classical economists’ multi-faceted picture of human psychology, to the early neoclassicals’ embrace of hedonism, to the postwar neoclassicals’ rejection of psychological foundations. The latter is particularly important, as it

remains the received view. In passing, we hope to have shown that, although behavioral economics as a subdiscipline is a rather recent development, attempts to tie economic theory to a psychologically plausible account of human judgment and decision making are as old as economics itself. In fact, both classical and early neoclassical economists were deeply interested in the psychological underpinnings of behavior.

A description of neoclassical economics would be incomplete, however, if it failed to mention that there is a set of auxiliary assumptions that tend to be used in conjunction with the theory. As we have seen, in the postwar view, neoclassical economics is extraordinarily general, in the sense that it makes no assumptions about motivation, preference formation and choice (beyond the proposition that observable choices satisfy certain axioms). The theory is so general, in fact, that by itself it has, arguably, no observable implications. For all practical purposes, therefore, the theory has to be combined with a series of auxiliary assumptions. Typically, these take the form of assumptions about the objects of preference, the characteristics of the budget set, and the properties of the preference ordering. For instance, in the case of choice among lotteries (which is the paradigm for many decision theory problems) it is standard to assume that people's subjective probabilities equal actual probabilities and that utilities are defined over wealth. Similarly, when it comes to intertemporal choice, it is standard to assume that individuals maximize the sum of utilities over time, discounted in the same fashion as financial markets discount cash flows. To say that such assumptions are standard does not, of course, mean that everybody adopts them, only that a large proportion of them do.

Our goal here is not to either defend or criticize the auxiliary assumptions, but to point out that the existence of standard auxiliary assumptions has generated some confusion about the nature of neoclassical theory. The confusion is between a weaker and a stronger conception of

neoclassical economics. The weaker conception insists that the research program is defined by the bare theory alone, and that auxiliary assumptions are external and incidental. The stronger conception insists that neoclassical economics must be seen as inclusive of the standard assumptions used to generate observable predictions. Unsurprisingly, defenders of neoclassical economics tend to rely on the weaker conception, whereas critics tend to rely on the stronger one. We return to these topics when we discuss (in section **Error! Reference source not found.**) how behavioral economics relates to neoclassical and other approaches.

3 PSYCHOLOGICAL APPROACHES DURING THE LATE NEOCLASSICAL PERIOD

When cognitive science finally emerged as an independent discipline, it did not have to be created out of thin air, as it were. In Gardner's words: "Interestingly, nearly all of the work that came to fruition in the postwar era was in fact built upon prior theoretical efforts – work often dating back to the beginning the century" (Gardner 1987, 16). Something very similar is true in the case of behavioral economics. Although behavioral economics emerged as an independent subdiscipline relatively recently, it could draw upon developments that can sometimes be dated back to the beginning of the twentieth century. (This is not, of course, to say that all behavioral economists in fact draw upon this work, even when they should.) Hence, behavioral economics –like cognitive science – can be said to have "[a] long past but a relatively short history" (Gardner 1987, 9).

In this section, we discuss some of the economists who, even in the midst of the relative hegemony of postwar neoclassical economics, and in many cases apparently independently of each other, insisted that neoclassical choice theory failed to accurately describe human choice behavior, and argued that the solution may lie in foundations with greater psychological

plausibility. Many of them took positive steps toward erecting economic theories on the basis of psychologically plausible foundations. Here, our goal is not to identify the exact position of the various authors, or how convincing those positions are, but to give a brief characterization of how, and why, they rejected the neoclassical view. As we will see, these economists refused to eschew psychological theorizing, mainly because they believed psychological insights would help them do better economics.

3.1 The institutionalists: Veblen, Mitchell and Clark

Some of the earliest and most vehement critics of ordinalist tendencies were the institutional economists of the early 20th century (Lewin 1996, 1294). In a 1914 survey, for instance, Wesley C. Mitchell criticized “recent writers” (like Pareto) who favored “non-intercourse with psychology” (Mitchell 1914, 1). In their writings, the institutionalists happily admit that hedonist psychology is flawed. In one famous passage, for example, Thorstein Veblen dismissed it this way:

The psychological and anthropological preconceptions of the economists have been those which were accepted by the psychological and social sciences some generations ago. The hedonistic conception of man is that of a lightning calculator of pleasures and pains, who oscillates like a homogeneous globule of desire of happiness under the impulse of stimuli that shift him about the area, but leave him intact (Veblen 1898, 389).

However, the institutional economists also believed that it would be a mistake for economists to ignore psychology. Thus, Mitchell hopes that the failure of hedonic psychology would encourage fellow economists to look for “a sounder psychological basis for our analysis” and that “economists will find themselves not only borrowing from but also contributing to psychology” (Mitchell 1914, 2-3). J. M. Clark (1918) echoes these sentiments, and adds that the economist cannot in the end avoid psychology. If the economist does refuse to let himself be inspired by the

psychologists' conception of man, Clark argues, "he will force himself to make his own, and it will be bad psychology" (Clark 1918, 4).

One reason why the institutionalists were interested in psychological underpinnings for human behavior was that they thought of institutions in psychological terms. As Mitchell (1910) put it: "Institutions are themselves conceived as psychological entities—habits of thought and action prevailing among the communities under observation" (Mitchell 1910, 112). The institutionalists appear to have read the psychology of the day quite closely, and used the knowledge they had acquired to generate economically relevant hypotheses. Thus, for instance, Clark (1918) explored the economic implications of stimulus-response psychology, studies of attention, and phenomena like habit formation. Mitchell (1914) ends his survey in the following manner:

It was because hedonism offered a theory of how men act that it exercised so potent an influence upon economics. It is because they are developing a sounder type of functional psychology that we may hope both to profit by and to share in the work of contemporary psychologists. But in embracing this opportunity economics will assume a new character. It will cease to be a system of pecuniary logic, a mechanical study of static equilibria under non-existent conditions, and become a science of human behavior (Mitchell 1914, 47).

Clearly, institutionalists like Mitchell believed that the incorporation of a more plausible psychology would make for better economics.

3.2 *The macroeconomists: Fisher and Keynes*

Other early forays into psychology appeared in the field of macroeconomics, especially in the context of monetary theory and the theory of the business cycle. Consider Irving Fisher, who is otherwise perhaps best known for his contributions to technical economics. Fisher is the author of the book *The Money Illusion* (1928), which aspired to explain phenomena like

business-cycle fluctuations in popular terms. Money illusion is a concept that Fisher may have invented (cf. Howitt 1998, 518), and which he used as early as 1913 (Fisher 1913, 135). It is defined as “the failure to perceive that the dollar, or any other unit of money, expands or shrinks in value” (Fisher 1928, 4). Fisher suggests that money illusion contributes to business cycle fluctuations because it conceals from view the principal cause – viz. the unstable dollar – of such fluctuations, and hence conceals the importance of stabilizing the dollar (Fisher 1928, 60 *idem*). At any rate, in Fisher’s view, money illusion makes business cycle fluctuations vastly more harmful than they otherwise would be.

Another macroeconomist well known for his forays into psychology is John Maynard Keynes. In his 2001 Nobel Prize lecture, George A. Akerlof went so far as to assert that “Keynes’ *The General Theory* [1936] was the greatest contribution to behavioral economics before the present era” (Akerlof 2003, 37). Indeed, Keynes departs from neoclassical orthodoxy in multiple ways. Consider the following famous passage, for example:

[A] large proportion of our positive activities depend on spontaneous optimism rather than on a mathematical expectation, whether moral or hedonistic or economic. Most, probably, of our decisions to do something positive, the full consequences of which will be drawn out over many days to come, can only be taken as a result of animal spirits—of a spontaneous urge to action rather than inaction, and not as the outcome of a weighted average of quantitative benefits multiplied by quantitative probabilities (Keynes 1936, 161).

This passage is interesting because he deviates from ordinalism in at least two ways: first, by suggesting that actual behavior is not adequately described by the expected utility model, and second by speculating about the motivation of economic behavior.

3.3 *The outlier: Scitovsky*

Returning to the micro level, a landmark study is Tibor Scitovsky's book *The Joyless Economy: The psychology of human satisfaction* ([1976] 1992). Scitovsky, who did early work in traditional welfare economics, gradually became disillusioned with the economists' hands-off approach to the study of preferences, which he found unscientific (Scitovsky [1976] 1992, xii-xiii). He starts out by writing that people's tastes and choices "are matters economists have always regarded as something they should observe, but must not poke their noses into" (Scitovsky [1976] 1992, xii). Rejecting this perspective, Scitovsky proposes instead to follow "behavioral psychologists" and "observe behavior ... in order to find ... the foundations of a theory to explain behavior" (Scitovsky [1976] 1992, xiii). Like the psychologists, he is not content to simply note differences in people's consumption patterns or "revealed preferences" but wants "to find the causes and explanation of the differences" (Scitovsky [1976] 1992, 28). Scitovsky is particularly impressed with the fact that psychologists support their theories with experimental data, and clearly believes that economists should do the same (Scitovsky [1976] 1992, xiii).

Scitovsky is particularly interesting because he made contributions to both positive and normative theory. On the positive side, Scitovsky draws on the psychology of motivation to argue that human beings (like other organisms) strive to maintain an optimal level of arousal (Scitovsky [1976] 1992, 24). Scitovsky argues that a great deal of behavior – for instance, our desire for novelty – can be understood in terms of this search for optimum arousal. He also argues that this process can explain the old paradox of why people would simultaneously buy insurance and lottery tickets; freely chosen uncertainty or risks (like those associated with buying lottery tickets) can help an individual approach the optimum level of arousal, Scitovsky argues,

while externally imposed, prolonged uncertainty (like that one would insure oneself against) would take the individual farther away from the optimum (Scitovsky [1976] 1992, 57-58). On the normative side, Scitovsky draws a distinction between comfort and pleasure. He argues that comfort has to do with absolute levels of arousal, whereas pleasure has to do with changes in arousal levels (Scitovsky [1976] 1992, 61). Thus, he infers that there is a tension between the pursuit of comfort and the pursuit of pleasure, in the sense that too much success at the former precludes success at the latter, and argues that people have a natural tendency to over-see comfort at the expense of pleasure (Scitovsky [1976] 1992, 62).

3.4 *The “old” behavioral economics: Simon and Katona*

In 1988, Peter E. Earl wrote: “There is no doubt that something called ‘behavioural economics’ has now begun to take off” (Earl 1988). The movement to which he referred has come to be called the “old behavioral economics,” to distinguish it from the modern developments which we will discuss shortly (Sent 2004, 740). According to Earl, the movement emerged from four different locations: Carnegie-Mellon University and the University of Michigan in the U.S., and the University of Oxford and the University of Stirling in the U.K. (Earl 1988, 3). Here, we will focus on the contribution of two towering individuals: Herbert A. Simon from Carnegie-Mellon University – to whom Gardner refers as “one of the founders of cognitive science” (Gardner 1987, 22) – and George Katona from the University of Michigan.

Simon reports that he was introduced to the social sciences by his uncle, a former student of the institutionalist economist John R. Commons (Simon [1978] 1992). Determined to infuse into the social sciences the same kind of mathematical rigor he felt had made the physical sciences so successful, Simon entered the University of Chicago in 1933 (Simon [1978] 1992). Simon ended up contributing to a range of fields, including economics, psychology and

computer science, and remained fiercely anti-disciplinary; indeed, Mie Augier and James G. March (2004) quote him as saying: “If you see any one of these disciplines dominating you ... you join the opposition and fight it for a while” (Augier and March 2004, 4). Simon’s critique of economic man, the standard economic model of behavior, as well as the outline of an alternative conception, are already present in his doctoral dissertation, defended in and published as *Administrative Behavior* (Simon [1947] 1957). Simon complained that economists “attribute to economic man a preposterously omniscient rationality” while psychologists following Freud tend to “reduce all cognition to affect” (Simon 1957, xxiii). As a result, Simon argues: “The social sciences suffer from a case of acute schizophrenia” (Simon 1957, xxiii).

His views on the topic are usefully developed in two entries in *The New Palgrave* dictionary of economics (Simon 1987a; 1987b). In his entry on “Behavioural Economics,” Simon starts by identifying the assumptions of neoclassical economics. He distinguishes two assumptions that tend to be explicit – viz. the assumption that “human goals and motivations are assumed to be given a priori in the form of a utility function” and that agents choose “that one of the alternatives that yields the greatest utility” – from a range of assumptions that tend to be implicit and which “are not necessarily maintained though all the different variants of the theory” – including assumptions to the effect that that agents have complete and certain knowledge or that they have a joint probability distribution (Simon 1987a, 221).^{vi} Then, he adds:

Behavioural economics is concerned with the empirical validity of these neoclassical assumptions about human behaviour and, where they prove invalid, with discovering the empirical laws that describe behaviour as correctly and accurately as possible. As a second item on its agenda, behavioural economics is concerned with drawing out the implications, for the operation of the economic system and its institutions and for the public policy, of departures of actual behaviour from the neoclassical assumptions. A third item on the agenda is to supply empirical evidence about the shape and content of the utility function (or of whatever construct will

replace it in a [sic] empirically valid behavioural theory) so as to strengthen the predictions that can be made about human economic behaviour (Simon 1987a, 221).

As Simon points out, behavioral economics is not defined in terms of a commitment to a given theoretical framework, but “as a commitment to empirical testing of the neoclassical assumptions of human behaviour and to modifying economic theory on the basis of what is found in the testing process” (Simon 1987a, 221).

It goes without saying that in Simon’s view, neoclassical models fail to accurately describe human choice behavior. He attributes this failure to “numerous cognitive limitations” and proposes that we use the term “bounded rationality’ ... to denote the whole range of limitations on human knowledge and human computation that prevent economic actors in the real world from behaving in ways that approximate the predictions of classical and neoclassical theory” (Simon 1987a, 222). Simon is aware of the fact that neoclassical choice theory is not intended as a correct description of the manner in which individuals come to a decision, but only “as an apparatus for predicting choice” (Simon 1987b, 267). By contrast: “Theories of bounded rationality is more ambitious, in trying to capture the actual process of decision as well as the substance of the final decision itself” (Simon 1987b, 267). Referring to the former theories as “substantive,” and the latter as “procedural,” Simon suggests that procedural theories are superior both because they can better predict and explain the decisions that are actually reached, and because they alone can shed light on decision making processes, which are of independent interest (Simon 1987b, 267).

Katona received his Ph.D. in psychology, but apparently as a result of experiencing hyperinflation in Germany in 1923, came to be interested in the psychological foundations of economic behavior (Katona 1975, viii). Drawing on his background in psychology as well as on several years experience with large-scale survey research on economic topics (Katona 1975, ix),

in 1951 he published a book called *Psychological Analysis of Economic Behavior* (Katona 1951).^{vii} The fundamental assumption of the book is that “economic processes stem directly from human behavior and that this simple but important fact has not received its due in modern economic analysis” (Katona 1951, iii). In particular, Katona is sharply critical of the use of the rationality assumption in neoclassical economics. As he writes:

Unlike pure theorists, we shall not assume at the outset that rational behavior exists or that rational behavior constitutes the topic of economic analysis. We shall study economic behavior as we find it. In describing and classifying different reactions, as well as the circumstances that elicit them, we shall raise the question whether and in what sense certain reactions may be called “rational.” After having answered that question and thus defined our terms, we shall study the fundamental problem: Under what conditions do more and under what conditions do less rational forms of behavior occur? (Katona 1951, 16).

Katona’s most fundamental critique of neoclassical economics, however, is that it fails to take proper account of the importance of intervening variables. Katona discusses statements like “consumer expenditures are a function of income” (1975, 5) and objects that “changes in discretionary expenditures [are a] function not only of ability but also of willingness to buy” (1975, 11). More generally, Katona argues that “motives, attitudes, and expectations of consumers and businessmen play a significant role in determining spending, saving, and investing and that modern psychology provides conceptual as well as methodological tools for the investigation of economic behavior” (Katona 1975, 4). Of course willingness to buy, as well as motives, attitudes and expectations, are all examples of the intervening variables of which he speaks. In Katona’s view, attention to such variables is critical: “Intervening variables are essential to psychological analysis because without them our description of economic behavior would remain incomplete, our understanding of behavior limited, and our predictions of future behavior incorrect” (Katona 1951, 31). When neoclassical economists fail to take proper account

of intervening variables, Katona argues, they are guilty of assuming that “human beings behave mechanistically,” i.e., that they “show invariably the same reactions to the same developments in the economic environment” (1951, 6).

As we have seen in this and the previous section, “old” behavioral economics took shape during the 1950’s and 60’s, during the heyday of postwar neoclassical theory. While the old behavioral economists – including Simon, Katona, as well as their colleagues and collaborators – differed in many respects, they also had a great deal in common. As Sent puts it:

Whereas mainstream economics started from a given utility function, old behavioral economics focused on discovering the empirical laws that described behavior correctly and as accurately as possible. While the neoclassical approach established a close connection between rationality and utility or profit maximization, old behavioral economics scrutinized the implications of departures of actual behavior from the neoclassical assumptions. And whereas mainstream economics started from given alternatives and known consequences, old behavioral approaches began with empirical evidence about the shape and content of the utility function (Sent 2004, 742).

The work discussed here helped inspire the foundation in 1972 of the *Journal of Behavioral Economics (JBE)*, intended to “(1) further knowledge of real world economic phenomena by integrating psychological and sociological variables into economic analysis and (2) promote interdisciplinary work” (‘Introduction,’ *JBE* 1972); in 1974 of the *Journal of Consumer Research*, and in 1982 of the Society for the Advancement of Behavioral Economics.

3.5 Discussion

In brief, even during the relative hegemony of postwar neoclassical theory, there were many economists who in various ways tried to build their economics on more psychologically plausible foundations. The above is only a brief sample. Additional work that could have been

mentioned in the context include James S. Duesenberry's *Income, Saving and the Theory of Consumer Behavior* (1949), Harvey Leibenstein's *Beyond Economic Man: A new foundation for microeconomics* (1976), and Richard Easterlin's work on happiness (e.g. 1974). These economists all rejected the ordinalist injunction against psychological theorizing, and many of them at least implicitly rejected the notion that economists should only try to describe behavior and not explain it. In spite of their number, and the fact that they in many cases were respected in the profession, these economists appear to have had little influence on the direction of economics as a whole. Insofar as these economists received any recognition for their work at the time, it appears to have been in spite of, rather than because of, their efforts to provide economics with psychologically plausible foundations.

It is worth mentioning that other economists joined the call for more psychologically plausible foundations. Such economists included Kenneth E. Boulding, who in a 1958 talk (published in 1961) discussed research trends in economics. Talking about development economics in particular, Boulding argued:

In spite of the moderate usefulness of what the economist has to say on this subject ... there is a cry for a cultural anthropologist or even a psychologist when the economist runs into sacred cows, extended families, traditional motivations, levels of achievement, and social morale, all of which may be more important to economic development than any of the traditional economic variables. We still await a true synthesis of the insights of economics with those of other social sciences in the area (Boulding 1961, 19).^{viii}

In his paper, Boulding predicts that there will be a movement toward what he calls “behavioral economics,” which in particular “involves study of those aspects of men’s images, or cognitive and affective structures, which are more relevant to economic decisions” (Boulding 1961, 21).

The economists discussed in this section – including the old behavioral economists – had some obvious successes. Keynes, of course, remains one of the most famous macroeconomists in

the history of the discipline. The widespread reliance on consumer confidence measures reflects Katona's ideas about the importance of expectations (Curtin 1982). Simon was awarded the 1978 Nobel Memorial Prize for "for his pioneering research into the decision-making process within economic organizations" (Bank of Sweden 1978). However, despite these successes, they had relatively little impact on the larger profession and, perhaps more surprisingly, only a limited impact on the development of the "new" behavioral economics. As it turns out, the rise of behavioral decision research was far more important.

4 THE "NEW" BEHAVIORAL ECONOMICS

According to Gardner, the first cognitive scientists recognized that the behaviorists had expressed legitimate concerns, e.g., regarding the naive reliance on introspection. Hence, they were cautious not to commit the mistakes committed by early twentieth-century psychologists and identified by the behaviorists. Nevertheless, they came to think that "adherence to behaviorist canons was making a scientific study of the mind impossible" (Gardner 1987, 12). Instead, Gardner writes, cognitive scientists came to the conclusion that "it is necessary to speak about mental representations and to posit a level of analysis wholly separate from the biological or neurological, on the one hand, and the sociological or cultural, on the other" (Gardner 1987, 6). Again, at greater length:

Cognitive science is predicated on the belief that it is legitimate – in fact, necessary – to posit a separate level of analysis which can be called the "level of representation." When working at this level, a scientist traffics in such representational entities as symbols, rules, images – the stuff of representation which is found between input and output – and in addition, explores the ways in which these representational entities are joined, transformed, or contrasted with one another (Gardner 1987, 38; cf. p. 383).

As we will see in this section, behavioral economists agree. They, too, believe that it is appropriate to talk about entities such as beliefs, emotions, and heuristics, which clearly are to be found at the level of representation. They think of these entities as at least partly responsible for the production of human behavior. As a result, behavioral economists believe that a deeper understanding of the former can help us better explain and predict the latter.

In this section we describe the emergence and establishment of behavioral economics as an independent subdiscipline of economics. It is not our intention to provide a survey of empirical results. Several such surveys already exist, including Matthew Rabin's (1998) 'Psychology and Economics,' published in the *Journal of Economic Literature* and Colin Camerer and George Loewenstein's (2003) 'Behavioral Economics: Past, present, future,' published as the introduction to the book *Advances in Behavioral Economics* (Camerer, Loewenstein and Rabin 2003). For easy access to classical and recent articles, there are several useful collections, including Daniel Kahneman, Paul Slovic and Amos Tversky (1982) *Judgment under Uncertainty: Heuristics and biases*, Loewenstein, Daniel Read, and Roy Baumeister (2003) *Time and Decision*, Kahneman and Tversky (2000) *Choices, Values and Frames*, and the above-mentioned *Advances* (Camerer et al. 2003). However, in sections 4.4-4.6, we do discuss, in a cursory fashion, three specific insights of behavioral economics that we deem especially important: loss aversion, nonlinear probability weighting and hyperbolic time discounting.

4.1 Behavioral decision research

From the point of view of modern – i.e. “new” – behavioral economics, the most important development was the emergence in the 1970s of a new branch of psychology called “behavioral decision making” (*BDM*) or “behavioral decision research” (*BDR*). *BDR* is often described as a direct consequence of the cognitive revolution. Thus, Reid Hastie and Robyn

Dawes (2001) identify two insights that emerged in the cognitive revolution, and which proved critical for the development of BDR. The first insight “is that many aspects of human thinking, including judgment and decision making, can be captured with computational models,” according to which we “compare, combine, and record ... mental representations” (Hastie and Dawes 2001, 9). The second insight is that properties of our cognitive apparatus “play major roles in our explanations for judgment and decision-making phenomena” (Hastie and Dawes 2001, 10). Thus, among other things, “the limited capacity of working memory is used to explain many departures from optimal, rational performance” (Hastie and Dawes 2001, 10).

Drawing on these insights, behavioral decision researchers, then, apply insights gleaned from the cognitive revolution to the topic of human judgment and decision making. As Hastie and Dawes summarize the take-home message of their 2001 textbook, and therefore by extension the whole field:

The most important finding is that diverse people in very different situations often think about their decisions in the same way. We have a common set of cognitive skills that are reflected in similar decision habits. But we also bring with us a common set of limitations on our thinking skills that can make our choices far from optimal (Hastie and Dawes 2001, 2).

A central focus of behavioral decision researchers, then, is to identify the common set of cognitive skills, their benefits and limitations, and to explore how they help produce observable behavior.

What truly distinguishes BDR from other approaches to human judgment and decision making, however, is that it studies judgment and decision making by taking as its starting point theories of rational decision. In the words of Dawes:

Basically, behavioral decision making is the field that studies how people make decisions. Because all types of people are making all sorts of decisions all the time, the field is potentially very broad. What has characterized

the field both historically and theoretically is the comparison of actual decision making with certain principles of rationality in decision making (Dawes 1998, 497).

The principles need not be derived from orthodox decision theory (cf. Hastie and Dawes 2001, 18-19) but in actual fact they often are. Dawes (1998) adds that merely random deviations would be of little interest, but that behavioral decision researchers have established that deviations are in fact “systematic and highly replicable in experimental settings” (Dawes 1998, 498). Much as visual illusions can often help to identify fundamental properties of visual perception, deviations from the ideal implicit in rational choice theory are not only interesting in their own right, but can potentially shed light on the basic mechanisms underlying human judgment and decision making.

These points are echoed by Baruch Fischhoff (1988), who writes that theories of rational decision making raise two main questions for psychologists: “(a) Do people perform the way that the models claim they should? (b) If not, how can people be helped to improve their performance?” (Fischhoff 1988, 156). Fischhoff goes on to explain how he sees the difference between the BDR and economics approaches to decision making. He writes that “economists have traditionally taken it as self-evident that people optimize their decisions... The goal of the empirically minded economist is, therefore, not to test the hypothesis that people optimize, but to determine what it is that people are trying to optimize” (Fischhoff 1988, 156). As this passage makes clear, BDR is very different from mainstream economics. Interestingly, however, there is a sense in which BDR would not have existed in the absence of the models of rational choice which characterize mainstream economics. It can be said that rational choice theory gave birth to BDR by providing a “hard target” – a theory that (in conjunction with widely used auxiliary hypotheses) made clear and crisp predictions that could be explored in laboratory and other settings – for its researchers (cf. Camerer and Loewenstein 2003, 5-7).

4.2 Tversky and Kahneman's biases and heuristics, prospect theory

It took the work of Tversky and Kahneman to bring BDR to the attention of economists. Several factors help explain their success. As psychologists, Tversky and Kahneman were well aware of psychological approaches to the study of human judgment and decision making. Yet, they had also mastered the formalism of economic theories of decision. For one thing, Tversky was a coauthor, along with David H. Krantz, R. Duncan Luce and Patrick Suppes, of the monumental *Foundations of Measurement* (1971). Measurement theory, as articulated in that work, had close historical and theoretical ties to economic theories of rational decision (cf. Angner forthcoming). Kahneman and Tversky's success, according to Matthew Rabin, resulted from the fact that “they are able and willing to address economists in standard economic language and venues” (Rabin 1996, 111).

Here, we will focus on two of Tversky and Kahneman's research projects: first, the heuristics and biases program – which achieved prominence with their seminal 1974 *Science* paper ‘Judgment under Uncertainty: Heuristics and biases’ (Tversky and Kahneman 1974) and a 1982 volume with the same title (Kahneman, Slovic and Tversky 1982), and second, Prospect Theory, presented in the extraordinarily influential 1979 *Econometrica* paper ‘Prospect Theory: An analysis of decision under risk’ (Kahneman and Tversky 1979) and further explored in another *Science* article titled ‘The Framing of Decisions and the Psychology of Choice (Tversky and Kahneman 1981). As David Laibson and Richard Zeckhauser (1998) argued: “[These] publications altered the intellectual history of economics; they brought the behavioral economics research program into the mainstream” (Laibson and Zeckhauser 1998, 19).

The thesis of the heuristics and biases paper paper is expressed in the following way: “This article shows that people rely on a limited number of heuristic principles which reduce the

complex tasks of assessing probabilities and predicting values to simpler judgmental operations” (Tversky and Kahneman 1974, 1124). Consistent with BDR in particular and cognitive science in general, the two authors are interested in the mechanism underlying human judgment and decision making. At the same time, however, they are interested in the conditions under which the mechanism leads us into error. As they put it: “In general, these heuristics are quite useful, but sometimes they lead to severe and systematic error” (Tversky and Kahneman 1974, 1124). Hence, each heuristic – including representativeness, availability, anchoring and adjustment, though generally useful, comes with characteristic biases that arise in special circumstances. The authors conclude by expressing the hope that a better understanding of the heuristics – that is, of the mechanism underlying people’s judgments – could improve judgments and decisions under conditions of uncertainty (Tversky and Kahneman 1974, 1131).

“Folk wisdom holds that ‘Prospect Theory,’” Laibson and Zeckhauser (1998, 8) write, “is the most-cited paper ever published in *Econometrica*.” The paper “presents a critique of expected utility theory as a descriptive model of decision making under risk, and develops an alternative model, called prospect theory” (Kahneman and Tversky 1979, 263). Central to their critique of orthodox decision theory is the observation of what they later called “framing effects,” in which “seemingly inconsequential changes in the formulation of choice problems caused significant shifts of preference” (Tversky and Kahneman 1981, 457). To explain framing effects and a variety of other anomalous phenomena, Instead, the two authors offer a theory in which, among other things, “value is assigned to gains and losses rather than to final assets,” (Kahneman and Tversky 1979, 263). This theory is capable of accommodating framing effects because what counts as a gain and what counts as a loss is relative to the frame; when going from one frame to another, the theory permits the agent to change his or her choice behavior. According to the

authors, prospect theory can accommodate a range of otherwise puzzling behavior, such as the fact that many people simultaneously gamble and buy insurance (Kahneman and Tversky 1979, 263).

4.3 *Thaler's anomalies*

Unlike Tversky and Kahneman, Richard Thaler received his Ph.D. in economics, from an institution he describes as “a University of Chicago farm club, and hardly a place to get interested in psychology” (Thaler 1991, xi). Apparently as a diversion from running regressions, Thaler started observing the manner in which people around him (especially, it seems, economist colleagues) made real-life decisions, and took note of the various ways in which they deviated from the ideal expressed by mainstream economic decision theory (Thaler 1991, xii). In the mid-1970s, he got to know Fischhoff, Slovic, Tversky and Kahneman, whose work (he felt) helped explain the anomalies he had observed (Thaler 1991, xii-xiii). Later, through his ‘Anomalies’ columns published in the widely distributed *Journal of Economic Perspectives* and collected in *The Winner's Curse* (1992), Thaler helped accelerate the awareness and acceptance of behavioral economics among mainstream economists (Loewenstein 1996, 351).

Thaler's first major contribution to behavioral economics was his 1980 paper ‘Toward a Positive Theory of Consumer Choice’ (Thaler, 1980). This paper argues that the “exclusive reliance on the normative theory [of consumer choice] leads economists to make systematic, predictable errors in describing or forecasting consumer choices” (Thaler 1980, 39). Drawing explicitly on the Kahneman and Tversky's work (1974, 1979), Thaler offers examples of “classes of problems where consumers are particularly likely to deviate from the predictions of the normative model” (Thaler 1980, 40). Like Tversky and Kahneman, Thaler is interested in

anomalies primarily as a means to an end, the end being the development of an empirically adequate descriptive theory of consumer choice (Thaler 1980, 40).

Anomalies discussed in the 1980 paper include the underweighting of opportunity costs, the failure to ignore of sunk costs, the influence of considerations of regret, self-control problems, and others. In a follow-up paper, 'Mental Accounting and Consumer Choice (Thaler 1985), Thaler developed "a new model of consumer behavior ... using a hybrid of cognitive psychology and microeconomics" (Thaler 1985, 199). Like Tversky and Kahneman, then, Thaler used cognitive psychology, first, to identify in what ways people's choices diverge from the predictions of rational choice theory, and second, to develop more empirically adequate theories. Thaler (1985) also continued to spell out the implications for economic decisions, in this case for marketing. The fact that Thaler spent so much time spelling out the implications of behavioral decision research, prospect theory, and so on, certainly helped bring the relevance of these developments home to economists and other social scientists with an interest in economic decisions.

4.4 Examples of current research

Reference-dependence: Standard economic theory assumes that consumers' preferences are invariant with respect to their current endowment or consumption. However, contrary to this assumption there is diverse evidence of 'reference-dependence' – i.e., that preferences depend strikingly on an individuals' 'reference point', which is usually equal to their current endowment. The notion of 'loss aversion' further specifies that people dislike negative departures from their reference point than they like positive departures, a pattern that can be depicted as a kink in the value function, or in indifference curves, at the current endowment point (Kahneman & Tversky, 1979; Tversky & Kahneman, 1991).

The combination of loss aversion and reference dependence has numerous implications, including predicting a phenomenon known as the "endowment effect" which is the observation that people tend to become extremely attracted to objects in their possession, and averse to giving them up, even if they would not have particularly desired the object had they not possessed it (e.g., Kahneman, Knetsch and Thaler, 1990). Loss aversion has proved a useful concept for making sense of field data (see Camerer, 2000). It has been used to explain asymmetries in demand elasticities in response to price increases and decreases (Hardie, Johnson, & Fader, 1993), the tendency for New York City cab drivers to quit early after reaching a daily income target, contrary to the prediction of conventional models of labor supply (Camerer et al, 1997), the tendency for investors to hold on to losing stocks longer than winning ones (Odean, 1998), the large gap between stock and bond returns (the "equity premium"; see Benartzi & Thaler, 1995), and the tendency for volume to diminish during downturns in housing markets (Genoseve & Mayer, 2001).

Non-linear probability weighting: The expected-utility EU model, which is the dominant model of risk-taking in economics, assumes that the value of a risky prospect is determined by the utility of its consequences weighted by their probabilities of occurring. EU is appealing, in part because it can be derived from an apparently reasonable set of axioms, most notably the independence (or "cancellation") axiom which entails that choice between gambles should not depend on events which lead to the same consequence with the same probability. However, many empirical studies of decision making under risk document violations of the patterns of behavior predicted by EU (see Starmer, 2000, for a review). Some of these violations can be explained by taking account of loss aversion and reference dependence (see prior subsection), but others are well explained by assuming a specialized probability weighting function that

overweights small probabilities and is insensitive to changes in probability in the midrange of probabilities. The most sophisticated of the new theories that allow for nonlinear probability weighting assume that probability weights are "rank-dependent"—outcomes are first ranked, then their probabilities are weighted in a way which is sensitive to how they rank within the gamble that is being considered (e.g., Tversky & Kahneman, 1992). The essential insight captured by most rank-dependent probability weighting schemes is that people tend to put disproportionate weight on (care about disproportionately) the best and worst outcomes of a risky prospect.

Hyperbolic time discounting: The discounted-utility (DU) model, which is the dominant economic model of intertemporal choice, assumes that people choose between intertemporal prospects by evaluating the utilities of their outcomes and "discounting" them according to their time of occurrence (see Loewenstein & Prelec, 1992, and Frederick, Loewenstein & O'Donoghue, 2002). DU incorporates a wide range of rather severe assumptions, such as that utility in each period depends only on consumption in that period, and that all forms of consumption are discounted in a similar fashion. Undoubtedly the most controversial assumption, however, and the one that has been tested (and found lacking) the most is the assumption that utilities are discounted exponentially, applying the same discount rate in each period. As it turns out, a simple hyperbolic time discounting function of $d(t) = 1/(1+kt)$ tends to fit experimental data much better than exponential discounting. Declining discount rates have been observed in experimental studies involving real money outcomes (Horowitz, 1992) and in field studies – e.g., a study in which Indian farmers made choices between amounts of rice that would be delivered at different points in time (Pender, 1996)

As first pointed out by Strotz (1955), hyperbolic time discounting implies that people will make relatively far-sighted decisions when planning in advance – when all costs and benefits will occur in the future – but will make relatively short-sighted decisions when some costs or benefits are immediate. Many authors such as Thaler (1981), Thaler and Shefrin (1981), and Schelling (1978) discussed the issues of self control and stressed their importance for economics, and Laibson (1997) accelerated the incorporation of these issues into economics by incorporating a hyperbolic discount function into an otherwise standard model of lifetime consumption-savings decisions. More recent papers by Laibson, Repetto and Tobacman (1998) and others have demonstrated that hyperbolic discounting potentially provides a better account than does conventional exponential discounting of various savings and consumption phenomena, such as different marginal propensities to consume out of different forms of savings, and the dramatic impact of liquidity constraints on savings. Finally, in a series of papers, O'Donoghue & Rabin (1999; 2001) have demonstrated the importance for behavior of whether hyperbolic time discounters while being impatient in the present are naïve or sophisticated about the fact that they will also be impatient in the future -- when the future becomes the present.

4.5 Discussion

In this section, we have argued that behavioral economics grew out of behavioral decision research (BDR) and gradually emerged as its own field. In turn, BDR was a direct result of the cognitive revolution. This heritage is clearly evident in the writings of contemporary behavioral economists. Thus, for example, many behavioral economists believe that social and behavioral science should aspire to reveal the actual causes of behavior. For instance, Camerer and Loewenstein (2003) write that “the ultimate test of a theory is the accuracy with which it identifies the actual causes of behavior” (Camerer and Loewenstein 2003, 4). On this view, both

the psychological plausibility of underlying assumptions, and the accuracy of predictions matter for the assessment of a theory. They matter because psychological plausibility and predictive accuracy are seen as indications that the theory “has pinned down the right causes,” not because they matter in and of themselves (Camerer and Loewenstein 2003, 4). There is little doubt that these methodological predilections reflect the cognitive psychology heritage. Indeed, it can be argued that behavioral economics – like cognitive science in general – owes its success in part to its use of entities at the level of representation.

It is worth noting that none of the authors discussed in this section criticize orthodox decision theory as a normative or prescriptive theory of decision. Hence behavioral economics do not reject the conception of rationality associated with neoclassical economics. Rather than modifying their normative theory in such a way that people’s behavior comes across as largely rational, behavioral economists tend to look at, e.g., framing effects as evidence that irrationality is systematic and widespread. According to Slovic, Dale Griffin, and Tversky (1990), for example, phenomena like preference reversals “represent deep and sweeping violations of classical rationality,” which implies that “it may not be possible to construct a theory of choice that is both normatively acceptable and descriptively adequate” (Slovic, Griffin, and Tversky 1990, 26).

5 THE METHODS OF BEHAVIORAL ECONOMICS

In Gardner’s view, one characteristic feature of cognitive science is its interdisciplinary approach. He writes that cognitive scientists “hope that, working together, they can achieve more powerful insights than have been obtained from the perspective of a single discipline” (Gardner 1987, 42). Among other things, research in cognitive psychology “has come to draw quite

naturally on evidence from psychology, neuroscience, and artificial intelligence – so much that disciplinary lines are beginning to blur” (Gardner 1987, 42). Behavioral economists, just like cognitive scientists, draw on evidence of many kinds and are comfortable using different methods to generate such evidence. In particular, unlike many postwar neoclassical economists, behavioral economists do not consider choice behavior the only kind of admissible evidence (though they consider it an important one). Gardner also suggests that the commitment to the interdisciplinary approach, and the use of different kinds of evidence, is in part what has made cognitive science so successful; quite arguably, the same thing is true for behavioral economics.

The fact that behavioral economists use a variety of methods makes them different from experimental economists, who define themselves on the basis of their endorsement and use of experimentation as a research tool. By contrast, behavioral economists are methodological eclectics; they define themselves, not on the basis of the research methods that they employ, but rather their application of psychological insights to economics. Experimental economists have made a major investment in developing novel experimental methods that are suitable for addressing economic issues, and have achieving a virtual consensus among themselves on a number of important methodological issues such as prohibitions on deceiving subjects. Although behavioral economists may not always endorse all of the methodological prescriptions of experimental economists (cf. Loewenstein 1999), when conducting experiments, behavioral economists have often found it expedient to play by experimental economists’ rules. The confusion of outsiders to the two fields is, therefore, understandable: historically, behavioral economists have relied on experiments to a great extent. Recent behavioral economics, however, has relied on an increasingly diversified and sophisticated set of methods that reflect its interdisciplinary heritage.

5.1 Hypothetical choices

Some of the earliest, and most important, papers in the field relied on subjects' responses to hypothetical choices – i.e., situations in which they were asked to imagine what they would do if presented with a particular decision. Consider, for example, Sarah Lichtenstein and Slovic's (1971) paper on preference reversal. One of the tasks used in that paper was described as follows: "S [the subject] was told he owned a ticket to play the bet and was asked to name a minimum selling price for the ticket such that he would be indifferent to playing the bet or receiving the selling price... Ss knew their decisions were 'just imagine'" (Lichtenstein and Slovic 1971, 47). Similarly for Kahneman and Tversky's seminal 'Prospect Theory' (1979) paper. The authors wanted to demonstrate a series of violations of expected utility theory, and introduced the topic in the following way:

The demonstrations are based on the responses of students and university faculty to hypothetical choice problems. The respondents were presented with problems of the type illustrated below.

Which of the following would you prefer?

A: 50% chance to win 1,000,

B: 450 for sure.

50% chance to win nothing;

... The respondents were asked to imagine that they were actually faced with the choice described in the problem, and to indicate the decision they would have made in such a case (Kahneman and Tversky 1979, 264).

These authors obviously assume that subjects – at least as some reasonable approximation – have an idea of how they would choose under specified counterfactual conditions.

The empirical basis of Richard Thaler's (1980; 1985) papers on mental accounting, which we contend helped kick off the field of behavioral economics, didn't present empirical data at all. The evidentiary basis of that paper consisted almost exclusively of "thought experiments": hypothetical cases of economic patterns of behavior that were inconsistent with

standard economic theory and which were intended to have face plausibility to the reader. The thought experiments were apparently inspired by Thaler's observations of the behavior of fellow economists, among others (Thaler 1991, xi-xii). For example, Thaler (1985) asked the reader to consider fictional scenarios such as the following: "Mr. S. admires a \$125 cashmere sweater at the department store. He declines to buy it, feeling that it is too extravagant. Later that month he receives the same sweater from his wife for a birthday present. He is very happy. Mr. and Mrs. S. have only joint bank accounts" (Thaler 1985, 199). Although it can be objected that the examples he provides are unpersuasive because they are not based on actual data, the examples were intended to be so obviously realistic as to be almost indisputable.^{ix}

Over time, the use of hypothetical choice studies came under attack from experimental economists, who complained that subjects in these experiments had no incentive to provide truthful, carefully considered, responses, and that some of the anomalous results uncovered by behavioral economists could be artifacts. Perhaps most famously, Grether and Plott (1979) undertook a test of the preference reversal phenomenon with the explicit intention to "discredit the psychologists' works as applied to economics" (Grether and Plott 1979, 623). Grether and Plott opened their paper by listing 13 different "theories" that intended to explain how the results could be "artifacts of experimental methods" (Grether and Plott 1979, 624). The theories range from #1 "Misspecified Incentives" to #13 "The Experimenters were Psychologists" (Grether and Plott 1979, 624-629). Ironically, the researchers ended up not only failing to discredit the effect, but concluding rather dramatically that:

Taken at face value the data are simply inconsistent with preference theory and have broad implications about research priorities within economics. The inconsistency is deeper than the mere lack of transitivity or even stochastic transitivity. It suggests that no optimization principles of any sort lie behind even the simplest of human choices and that the uniformities in human choice behavior which lie behind market behavior may result

from principles which are of a completely different sort from those generally accepted (Grether and Plott 1979, 623).

Although Grether and Plott's effort to discredit the psychologists' work failed, worries about the validity of hypothetical choices have remained.

5.2 *Experiments with actual outcomes*

Some behavioral decision researchers were not satisfied with hypothetical choices alone, and aspired to test their results using experiments with actual outcomes. Even in the original preference reversal paper, Lichtenstein and Slovic (1971) reported the results of an experiment designed to test “whether the predicted results would occur under conditions designed to maximize motivation and minimize indifference and carelessness,” and where among other things, “[the] bets were actually played and *Ss* were paid their winnings” (Lichtenstein and Slovic 1971, 51). Still, reversals did not go away. Subsequently, Lichtenstein and Slovic (1973) replicated the results at a Las Vegas casino, where a croupier served as experimenter, professional gamblers served as subjects, and winnings and losses were paid in real money.

Over time, experiments involving real outcomes started replacing hypothetical choices as the “gold standard” for research in behavioral economics. This shift occurred in part as a response to the critique of experimental economists like Grether and Plott, as psychologists were determined to show that their results would survive the exposure to experiments with real outcomes. One prominent such study was Kahneman, Knetsch and Thaler's (1990) investigation of the endowment effect. The three authors report the “results from a series of experiments involving real exchanges of tokens and of various consumption goods” (Kahneman, Knetsch and Thaler 1990, 1328). Subjects traded items such as Cornell coffee mugs and folding binoculars for induced-value tokens, i.e., tokens that can be exchanged for real money at the conclusion of the

experiment. The experiments allowed the authors to conclude that the endowment effect is instant, in the sense that “the value that an individual assigns to such objects ... appears to increase substantially as soon as that individual is given the object,” but also that “the endowment effect can persist in genuine market settings” (Kahneman, Knetsch and Thaler 1990, 1342-1343). Their paper did not, however, spell the end of debate about the validity of the endowment effect; Charles Plott and collaborators (date) in a recent series of experiments, find a combination of experimental conditions which seem to eliminate the effect.

Beyond simply switching to experiments involving real outcomes, behavioral economists have also probed whether and when, in fact, eliciting hypothetical versus real choices mattered. The question of whether – or under what conditions – empirical results are robust under changes in the experimental method has generated a small literature of its own. After reviewing “74 experiments with no, low, or high performance-based financial incentives,” Camerer and Hogarth (1999) concluded: “The modal result is no effect on mean performance (though variance is usually reduced by higher payment)” (Camerer and Hogarth 1999, 7). However, some studies have found substantial differences between hypothetical and real outcomes. For example, Neill et al. (1994) elicited from subjects either real or hypothetical buying prices for a range of goods and found that hypothetical prices tended to be higher, as if it is easier to part with an imaginary dollar than with a real one (although, see Johannesson, Lijas & O’Conor, 1997 for contrary results). Other studies have found dramatic differences in behavior as a result of the magnitude of stakes employed in an experiment. Thus, for example, Parco, Rapoport and Stein (2002) found that people played the centipede game cooperatively when the stakes were small, but that behavior deteriorated toward the Nash equilibrium, which entailed immediate defection and massive opportunity losses for both players, when the stakes were increased.

Other research shows that people seem to be particularly bad at reporting on how they would behave in a situation different from the one they are currently in. The second author and colleagues (e.g., Loewenstein and Adler, 1995; VanBoven, Dunning & Loewenstein, 2000; VanBoven, Loewenstein & Dunning, 2003), for example, have shown that people who are not endowed with a mug massively underestimate their minimum selling price for a mug, if they possessed one, and those with mugs overestimate the amount they would pay to acquire one if they didn't already own one.

5.3 *Field research*

During the last decade or so, behavioral economists have increasingly relied on data gathered “in the field.” Thus, Linda Babcock, Xianghong Wang, and Loewenstein (1996) studied how social comparisons influence teacher contract negotiations. The authors relied on both survey data – gathered by administering questionnaires to negotiators from 75 school districts – about social comparisons, and field data on school district and community characteristics (Babcock, Wang and Loewenstein 1996, 8-10). In one of the most well-known studies of its kind, Camerer, Babcock, Loewenstein and Thaler (1997) explored the labor supply of New York City cabdrivers, using data from “trip sheets” – that is, forms where drivers record the time passengers were picked up and dropped off, as well as the amount of the fares, and from the cabs' meters, which automatically record the fares (Camerer et al. 1997, 412-413). Genoseve and Mayer (2001) studies patterns of sales of Boston condominiums during a downturn in prices and found, consistent with loss aversion, that those who faced the prospect of selling at a loss relative to what they had paid held out longer before selling at a particular price. Choi, Laibson and Madrian (2005) examined the behavior of investors in the aftermath of the Enron debacle in which employees of that company invested in their own company's stock and ended up losing

their retirement nest eggs along with their jobs when their company went bust. They were unable to detect any impact of these events on employees' ownership of their own companies' stocks at other companies.

Behavioral economists' excursion into the field has been driven in large part by concerns about the *external validity* of laboratory experiments, whether they involve hypothetical or real outcomes. Because the laboratory context is inevitably different from real-world decision situations – e.g., in context, information, and stakes – there is reason to think that people might make different decisions in the lab than they make in the real world. For some types of issues, such as how people behave in Internet auctions, experiments may have high external validity since all of these factors are likely to be relatively similar. For other questions, however, such as how much people save, how they invest their money, or how long or hard they work on a day, laboratory studies are may not be quite as reliable.

Field studies come in different, albeit often not sharply distinguished categories. The simplest studies are 'observational' studies in that investigators attempt to draw inferences from observations of naturally occurring behavior, whether at the individual or market level. In this sense, both of the studies mentioned above – the one about teacher contract negotiations and the one about New York City cabdrivers – were observational. The Achilles heel of such studies derives from their correlational nature, which lowers their *internal validity* – the ability to draw confident causal conclusions on the basis of empirical observations. It is notoriously hard to definitively infer causation from correlation, so such studies are inevitably beset by problems of potential confounds as well, often, as the problem of reverse causality.

In response to worries about internal and external validity, some behavioral economists have started looking for natural experiments – situations in which it is possible to observe the

impact of a quasi-exogenous change in events – or conducting field experiments. Examples of the former are the many studies examining the impact of changes in defaults and other features of company sponsored savings plans on employee saving behavior (e.g., Madrian & Shea, 2001). Participants in these studies are not randomly assigned to one treatment or another; but it is often possible to measure the effect of the changes with some degree of confidence by comparing savings behavior before and after the change or by comparing the behavior of people introduced to the change at different points in their tenure at the company.

Although they are not without problems, however, clearly the new gold standard for empirical evidence has become the randomized field experiment (see Harrison & List, 2004). Because field experiments take place under conditions highly similar or identical to those of real-life decisions, they can be argued to have high external validity. At the same time, because they involve randomized assignment to test and control groups, they make it easier to draw causal conclusion and therefore arguably have high internal validity. In one field experiment Duflo and Saez (2002) assigned a random sample of employees in a subset of departments to be offered a \$20 payment for attending an informational fair dealing with savings. Enrollment was significantly higher in department where some individuals received the monetary inducement to attend the fair than in departments where no one received the inducement. However, increased enrollment within these treated departments was almost as high for individual who did not receive any monetary inducement as it was for individual who did, demonstrating the influence of social information. In another study, List (2003) assigned novice and experienced baseball card traders to conditions in which they were given the opportunity to buy or sell various sports memorabilia. The study found that inefficiently low number of trades occur for naive traders, but that the same effect did not occur for traders who had significant amounts of experience.

Yet another field experiment, conducted by a group of economists who have been attempting to apply insights from behavioral economics to issues of economic development, Ashraf, Karlan and Yin (2006) investigated the impact of offering a "deposit collection" service offered by a rural bank in the Philippines that made it easy for individuals to deposit small amounts of money in a savings account on savings behavior. The authors found that those offered the service saved 188 pesos more (which equates to about a 25% increase in savings stock) and were slightly less likely to borrow from the bank. Field experiments, although still relatively rare in behavioral economics, are probably the fastest increasing category of study.

5.4 Process measures, including fMRI

To some extent, behavioral economists have also used what psychologists refer to as “process measures” – i.e., methods that provide hints about the cognitive and emotional processes underlying decision making. Although behavioral economists are acutely aware of the pitfalls of process measures, most notably the limitations of verbal accounts of the causes of one’s own behavior (see Nisbett and Wilson 1977), they have not rejected the use of process measures. For example, several behaviorally oriented game theorists have used computerized ‘process tracing’ software to assess what information players in games are using to make decisions – e.g., whether players in shrinking pie games choose to look at payoffs in the last round, as they would if they were solving the game using backward induction (they do not) (Camerer et al., 1994; Costa Gomes, Crawford & Broseta, 2001; Johnson et al., 2002).

Certainly the most exotic of the process measures currently being used, however, are brain scans, typically using functional magnetic resonance imaging (fMRI), which allows researchers to examine, albeit crudely, which parts of an individual's brain are activated in response to a task or decision. Although brain imaging has only been a part of behavioral

economics for a few years at the time of writing this paper, imaging methods have already been applied to a diversity of economic tasks, including decision making under risk and uncertainty, intertemporal choice, buying and selling behavior and strategic behavior in games (see Camerer, Loewenstein and Prelec 2005). Moreover, even more exotic neuroscience methods are beginning to be employed. For example, Ernst Fehr and colleagues (Knoch et al., 2006) studied the impact on behavior of responders in the 'ultimatum game' of disabling a part of subjects' brains called the Right Dorsolateral Prefrontal Cortex, using a tool called Transcranial Magnetic Stimulation (TMS). Interestingly, this study produced results that were seemingly opposite to an earlier study that examined behavior in the same game using fMRI (Sanfey et al., 2003), underlining ambiguities in the interpretation of neural data as well as the need to approach the same problem using multiple methods.

5.5 *Discussion*

In this section we have seen how behavioral economists, like cognitive scientists, draw on many kinds of evidence and are comfortable using different methods to generate such evidence. In particular, while behavioral economists are suspicious of introspective data, they do not consider choice behavior the only kind of admissible evidence. In fact, behavioral economics has been successful in part because it has drawn inspiration from theoretical and methodological developments in neighboring fields, succeeded in integrating methods from these fields, and exploiting data gathered from a variety of sources. By not connecting itself to any particular methodology, but rather exploiting new research methods as they come on line and attempting to use the best method to address whatever problem is being considered, behavioral economics has helped to ensure its own vibrancy.

6 CURRENT DIRECTIONS

Since its emergence as an independent subdiscipline, behavioral economics has seen a remarkable expansion. In light of this fact, it would be impossible to accurately describe current research in but a few paragraphs. Nevertheless, we want to say a few words about some of the latest developments in the field. Some of the new developments can be tied to the loosening of ties between behavioral economics and behavioral decision research and the importation of insights from other subfields of psychology. Here we discuss two: the emergence of neuroeconomics and the increased interest in the role of affect in economic behavior. The other major new development that we describe in this section is the emergence of various forms of 'light paternalism', which takes behavioral economics in new applied directions.

6.1 Neuroeconomics

As already touched upon in the section on methodology, neuroeconomics involves using the emerging array of tools developed by neuroscientists to study the neural underpinnings of economic behavior. Neuroeconomists have already conducted studies in which subjects' brains are scanned while they engage in mainstay behavioral economics tasks, such as the ultimatum game (Sanfey et al., 2003;), decision making under risk (Tom et al., 2006) and uncertainty (Hsu et al., 2005) and intertemporal choice (McClure et al., 2004), as well as more traditional economic behaviors such as deciding whether to purchase consumer goods (Knutson et al., 2007). These studies have generally come to similar conclusions, namely that decision making can be understood, not at as a matter of implementing existing preferences, but rather as the resolution of interaction, and often competition, between different specialized neural systems (see, e.g., Sanfey et al., 2006).

Neuroeconomics not only encompasses empirical work using neuroscience methods, but also involve importing insights from neuroscience to refine economic models of behavior. Again, perhaps the most important of these insights is that behavior, including economic behavior, results from the interaction of multiple interacting specialized neural systems. Thus, for example, Thaler and Shefrin (1981) proposed a multiple-self model in which a person's behavior is directly controlled by a series of myopic "doers" who maximize short-run satisfaction, but the behavior of the doers is itself influenced by a farsighted "planner" who maximizes the discounted sum of the doers' utilities (see Fudenberg & Levine, 2004, for an updated version of such a model). Bernheim and Rangel (2004) build a dual-process model of addiction which assumes that the brain can operate in one of two modes, a "cold mode" or a "hot mode". In the cold mode, the person makes sound, deliberative decisions with a broad, long-term perspective. In the hot mode, the person's decision-making is influenced by emotions and motivational drives. Which mode is triggered depends (stochastically) on environmental conditions, which in turn might depend on past behavior (e.g., if you choose to go to a party tonight rather than stay home, you increase the likelihood of experiencing a craving for alcohol tonight). Benhabib and Bisin (2002) assume that a person's behavior can be determined either by "automatic processes" or by "controlled processes." In their formulation, automatic processes are initially allowed to determine behavior, but controlled processes get activated whenever the costs from letting the automatic processes carry on become too large. They apply this framework to understanding saving behavior and describe how its predictions differ from those in saving-consumption models with hyperbolic discounting.

Although neuroscience methods and ideas have up until now influenced economics in a fairly incremental fashion, it is possible that their influence will ultimately prove to be much

more radical (cf. Camerer, Loewenstein, and Prelec 2005, 10). Incremental approaches take as their starting point orthodox decision theory and favor piece-meal, step-wise change. The process can be described in the following way (cf. Camerer and Loewenstein 2003, 7). First, identify normative assumptions or models that are ubiquitously used by economists, such as Bayesian updating, expected utility, and discounted utility. Second, identify anomalies—i.e. demonstrate clear violations of the assumption or model, and painstakingly rule out alternative explanations, such as subjects' confusion or transaction costs. Third, use the anomalies as inspiration to create alternative theories that generalize existing models. A fourth step is to construct economic models of behavior using the behavioral assumptions from the third step, derive fresh implications and test them. Many of the most important developments in behavioral economics – like prospect theory – were the result of an incremental approach.

By contrast, radical approaches try to improve the predictive power and explanatory adequacy of current theory by starting from scratch. Examples of radical approaches proposed outside of the realm of neuroeconomics include reason-based decision theory (Shafir, Simonson, and Tversky 1993) and case-based decision theory (Gilboa and Schmeidler 1995). Though radical approaches have not yet scored any successes comparable to prospect theory, it is still too early to judge this research program. Neoclassical economics has dominated the economic scene for almost as long as classical economics dominated before it, so the time may be ripe for a new revolution. If so, behavioral economics, and perhaps its neuroeconomic variant, show promise of identifying the direction for such a transformation.

6.2 Affect

In light of the emphasis in this review on the influence of the cognitive revolution on behavioral economics, it is important to draw attention to a major trend in behavioral economics

that diverges from this pattern that focuses on affect rather than cognition. The role of affect in decision making and behavior more generally is a major focus of research in decision research, psychology and neuroscience. In decision research, a number of new lines of research have begun to draw attention to the important role of affect in judgment and choice (Lerner & Keltner, 2001; Loewenstein, 1996; Loewenstein & Lerner, 2003; Loewenstein et al., 2001; Mellers, Schwartz & Ritov, 1997; Slovic et al, 2002;). Whereas traditionally decision theorists tended to focus on more cognitive types of errors, such as judgmental biases, framing effects, hyperbolic time discounting and nonlinear probability weighting) as the main sources of suboptimality in decision making, the new research is drawing new attention to, and providing new evidence for, the idea that affect can distort decision making – that people can behave self-destructively in the "heat of the moment" (e.g., Ariely & Loewenstein, 2005). Indeed, the new research is also pointing to the conclusion that many biases that had earlier been viewed in cognitive terms, such as nonlinear probability weighting (Loewenstein et al., 2001; Rottenstreich & Hsee, 2001) or hyperbolic time discounting (Loewenstein, 1996; McClure et al., 2004) may in fact reflect the influence of affective factors.

Parallel developments have been occurring in psychology, with a large amount of work in the field of social psychology focusing on the role of emotion in behavior (e.g., Epstein et al., 1992; Sloman et al., 1992; Wilson, Lindsey & Schooler, 2000; Zajonc, 1980, 1984). And similar developments are occurring in neuroscience, with that field showing signs of splitting into two subfields, one focusing on 'cognitive neuroscience' and the other on 'affective neuroscience' (Damasio, 1994; Panksepp, 1998; LeDoux, 1996; Rolls, 1999) and also decision researchers highlight the role that affect plays in decision making and lend new support to the ancient intuitions about human behavior.

In an indication of the vibrancy of the field, a sign that behavioral economics is responsive to new developments in the fields it draws on, in both empirical work and in theory-development, a number of behavioral economists have been incorporating insights from the new research on affect into their work (see, Rick & Loewenstein, 2007 for a recent review). Whether it be for the purpose of understanding problems of self-control, destructive conflict, market gyrations or gambling behavior, there is a growing recognition among economists that large domains of economic behavior are going to fall outside of the range of economic models unless economists begin to get a grip on the role of emotions in behavior.

6.3 Light paternalism

Although behavioral economics began as a descriptive enterprise, it is only a small step from the conclusion that people commonly behave in suboptimal ways to the view that it might be beneficial to help people improve their decision making. Indeed, a whole program of what could be called "light paternalism" (Loewenstein & Haisley, forthcoming) has gained prominence in the last few years. The essence of the different variants of light paternalism is to help people make better choices – choices that better serve the chooser's interests – without restricting autonomy or freedom of choice (see Camerer et al., 2003; Thaler and Sunstein 2003).

In their treatment of paternalism, for example, Sunstein and Thaler note that in many situations paternalism cannot be avoided. They illustrate the point with the hypothetical case of a company cafeteria manager who has the option of placing healthy items before unhealthy items in the food line or doing the reverse, but does not have the option of doing neither. Sunstein and Thaler argue that in such situations it make perfect sense for managers to adopt the option that they believe is better for employees – namely placing the healthy food ahead of the unhealthy food.

Camerer et al., make a different, but related point. In many situations, they argue, it is possible to craft policies that will benefit people if they do make mistakes, but will not hurt people who are fully rational. An example would be defaults on 401k plans. If it is beneficial to invest in a 401k plan, but people tend to stick with the status quo, then it may make sense to change the usual default from not contributing (with the possibility of signing up) to contributing (with the possibility of opting out). If people are, contrary to the dictates of conventional economics, influenced by the default option, then changing the default could potentially benefit them; if they are not influenced by the default, then changing it will have no effect on behavior and little if any cost. Such policies, one can see, not only provide benefits to agents who make mistakes without hurting those who are making a deliberate decision, but should also appeal to both economists who do and who do not believe in rationality. Economists who believe in rationality will see them as little more than a low cost nuisance, while economists who believe that people are less than perfectly rational will see them as beneficial.

Perhaps the most important and comprehensive paternalistic intervention to promote 401(k) contributions is the "save more tomorrow" (SMarT) program, designed and tested by Thaler and Benartzi (2004). Employees were approached and asked if they would increase their 401(k) contribution rates at the time of their next pay raise. Since the contribution rate does not increase until after a raise, employees do not perceive the increased savings as a cut in take-home pay. Once employees sign up for the plan, they remain enrolled until they reach the maximum contribution rate or until they opt-out, playing on the status quo effect – the reluctance of people to change patterns of behavior once they have been established. A test of this intervention found that enrollment was very high (78%), that very few who joined dropped out, and that there were dramatic increases in contribution rates (an increase from 3.5% to 11.6% over 28 months).

In their paper introducing the notion of libertarian paternalism, Thaler and Sunstein (2003:175) state that "a policy counts as 'paternalistic' if it is selected with the goal of influencing the choices of affected parties in a way that will make those parties better off," and then continue, "We intend 'better off' to be measured as objectively as possible, and we clearly do not always equate revealed preference with welfare." But what does it mean to measure "better off" "objectively"? As Sunstein and Thaler hint, preference-based, measures of welfare are not up to the job because they equate utility with preference, and hence automatically assume that anything a person voluntarily chooses to do must be welfare-enhancing. Clearly, it doesn't make sense to assess whether someone is committing an error using a measure that is premised on the assumption that people don't commit errors.

How, then, should one evaluate the desirability of different patterns of behavior or the success of policies designed to make behavior more self-interested? A wide range of approaches have been proposed. For example, Kahneman (1999, 15) advocates the use of "objective happiness" – the time-integral of momentary happiness – as a welfare criterion. Most contributions to behavioral welfare economics, however, are best understood as using a modified version of the standard preference-based criterion. Bernheim and Rangel (forthcoming), in contrast, advocate using a choice-based approach, but limiting the range of choices that are treated as self-interested – i.e., basing judgments of welfare on a subset of choices that are deemed to be self-interested.

6.4 Behavioral economic imperialism

In an essay unapologetically titled "Economic Imperialism," Edward Lazear (2000) trumpeted the expansion of neoclassical economics into such diverse fields as law, political

science, history, and even demography. If neoclassical boosters such as Lazear, have reason to celebrate, then behavioral economists do as well. In its relatively short lifetime (arguably 26 years) behavioral economics has influenced a wide range of subtopics of economics and allied fields, such as behavioral law and economics (Jolls, Sunstein & Thaler, 1998; Sunstein, 2000) to behavioral finance (e.g., Shleifer, 2000) behavioral development economics (Mullainathan, forthcoming), behavioral public finance (McCaffery & Slemrod, 2006), behavioral game theory (Camerer, 2003), and behavioral macroeconomics (Akerlof, 2003). All of these are booming areas of research that not only extend the influence of the ideas coming out of behavioral economics, but also throw back insights and findings that enrich the foundations of the basic science core of the field.

6.5 Discussion

Although the cognitive revolution, in effect, provided the impetus that sent behavioral economics 'into orbit', the field has maintained its vibrancy by drawing on other sources of inspiration, notably input from research on neuroscience and affect. It has also increased its broader relevance by pioneering new approaches to public policy, most notably those based on different forms of 'light' paternalism. Finally, in a pattern much like that of rational choice theory, but compressed into a much shorter period, behavioral economics has begun to export its insights to allied fields which have not only increased the range of applications but also thrown insights and research findings back to the core of the field.

7 CONCLUSION

In this chapter, we have argued that behavioral economics – the attempt to increase the explanatory and predictive power of economic theory by providing it with more psychologically plausible foundations – is best seen as a branch of cognitive science. Historically, as we have seen, behavioral economics was a direct result of the cognitive revolution. This heritage is reflected in behavioral economists' willingness to operate on the level of representation (as Gardner put it), that is, by their willingness to explain behavior by reference to cognitive, emotional and affective states, among other things. The cognitive science heritage is also mirrored in behavioral economists' interdisciplinary approach, in their efforts to render their economics consistent with the best available psychology, in their admission of evidence of many kinds, and their use of different kinds of method. In this concluding section, we want to add a few words about current approaches within behavioral economics, and to add some remarks about the advantages and disadvantages of the enterprise.

In the above, we have suggested that the success of behavioral economists is in part due to the fact that they have been able to integrate evidence drawn from a variety of sources, and that they have taken advantage of the possibility of postulating entities on the level of representation. The references to unobservable entities – like cognitive, affective, and motivational states – may also be the biggest disadvantage of behavioral economics, in that it remains unclear what the exact ontological and epistemological status of these entities are. However, there is no reason to think that references to unobservables entities are more problematic in behavioral economics than in cognitive science in general. Moreover, given the immense success of cognitive science in general, it seems justified to think of the ontological and epistemological status of unobservable entities as a fact to be explained rather than as a problem

to be solved. So far, behavioral economics – just like cognitive science in general – comes across as a vibrant subdiscipline of economics, and one that is likely to have a major impact on the face of economics.

NOTES

ⁱ Cf. Matthew Rabin (2002, 657-658) and Esther-Mirjam Sent (2004, 735-737).

ⁱⁱ There are exceptions, of course, including Esther-Miriam Sent (2004), Don Ross (2005) and Alex Rosenberg (manuscript).

ⁱⁱⁱ Although Gardner's account of cognitive science is hardly the last word on the matter, for present purposes it will do just fine.

^{iv} A reference has been omitted.

^v Note that in practice that many if not most data sets used by modern neoclassical economists include people's self-reports, e.g., regarding income, spending, work hours, willingness-to-pay, willingness-to-accept, etc.

^{vi} The latter are the ones we called "auxiliary assumptions" above.

^{vii} The central theses of the book also appear in an earlier paper in the *Journal of the American Statistical Association* (Katona 1947).

^{viii} A footnote has been omitted.

^{ix} A strange occurrence was the publication of a paper titled "A problem of evidence in the work of Richard Thaler" (find cite). Encountering this title, one of the authors worried that the paper claimed to expose some type of fraud on the part of, arguably, the founder of behavioral economics. However, in fact the 'problem' turned out to be nothing more than Thaler's use of hypothetical choice data – a point he acknowledges from the outset.

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